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AUTHOR Adams, Stuart
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ABSTRACT

Martinson's provocative article in Public Interest (Spring, 1974), denying efficacy in prisoner reform, singled out one of the uncertainties in correctional research. In their totality, these uncertainties embrace not only rehabilitative programs but also the method, theory, and organization of correctional research. To comprehend the status and effectiveness of correctional evaluation, we need to clarify such issues as (1) the relative efficacy of correctional treatments, (2) the importance of system models in evaluation, (3) extent to which research should strive for rigor in design, (4) importance of the theory in treatment and research, (5) proper location of research effort, (6) the place of advocacy in reasearch, (7) preferred training for researchers, and (8) optimal administrative styles for correctional administrators. Logical and empirical considerations suggest that prisoner reform is as efficacious as other varieties of social reform; that system models of evaluation are under-used; that "weak" research designs impact correctional policy as heavily as "strong" designs; that "no theory" may be more useful than "formal" theory; that in-house researchers impact correctional policy more heavily than outside researchers; that advocacy is preferable to neutrality in applied correctional research; that "academic" orientation in applied researcher training may be dysfunctional; and that the impact of research is greater under "experimental" than under "committed" or "trapped" administrators. (author)

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EVALUATIVE RESEARCH IN CORRECTIONS: THE UNCERTAIN ROAD*

Stuart Adams
Criminal Justice Consultant
Berkeley, CA 94709

Sages long have told us that admission of ignorance is the beginning of wisdom. We can probably say the same of admission of uncertainty. And if present uncertainty is any measure of future wisdom, then correctional researchers face an unprecedented era of learning. For the dimensions of our uncertainty are enormous indeed.

For many observers, the major uncertainty emerged nearly two years ago from Martinson's provocative article in Public Interest (1974). The doctrine that "almost nothing works" in correctional treatment badly shook administrators and researchers alike. The shock grew when a hard, "nothing works" version of the doctrine was enunciated later on CBS' "Sixty Minutes" program. Martinson has since repudiated the hard version of the doctrine (1975), but the shock remains.

So does the uncertainty. Some researchers are beginning to note fallacies in Martinson's position, but policy-makers show a strong interest in the "nothing works" concept. The tide toward punishment and incapacitation is impressive, and we now have one more issue in correctional treatment research to confuse us.

The Bases of Uncertainty

What are these issues, and what are their implications for

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the correctional researcher? Let me list a few of the better known ones, and then follow up with a discussion of their present status and their meaning for the future. I speak more as an interested observer than as an invincible authority, and if I am shown wrong tomorrow I shall try not to be too defensive. As for the issues, let's limit ourselves to eight:

1) Evaluations of correctional treatment show, generally, little effect; punishment is undoubtedly more effective;
2) We should concentrate more on changing the correctional system than on changing the correctional client;
3) We need better, more rigorous research, with emphasis on controlled experimentation;
4) Better correctional theory is required, and programs should be set up to test formal theory;
5) Location of the research effort is important; for quality and objectivity, the academic setting appears best;
6) Researcher orientations need changing; we need more activism and advocacy among researchers;
7) Researcher training needs overhauling; academic Ph.Ds. tend to persist in re-doing their theses in agency settings;
8) A new breed of agency administrators is required -- persons who operate under an experimental rather than a committed perspective.

Making Sense of the Issues

In our limited time, we can hardly expect to develop and resolve the arguments that center on these issues. Nevertheless,

it should be helpful to comment tentatively on recent thought in each area and to speculate on where this thinking seems to lead.

1) The "Almost Nothing Works" Doctrine: Martinson was not the first to assert that with few exceptions, correctional treatments are inefficacious. Sophia Robison (1961) said it 13 years earlier and Walter Bailey (1966) said it eight years earlier. However, Martinson, by saying it at greater length, more vehemently, on a wider variety of media, with right hand held solemnly on a 736-page document of scientific reports, has had far greater impact. He has thrown the correctional community into "Martinson-shock," inducing it to turn -- at least momentarily -- from rehabilitation toward deterrence, punishment, and incapacitation as more worthy criminal justice goals (Morris, 1975; Wilson, 1975).

Not everyone agrees with Martinson. Two formal critiques of The Evaluation of Correctional Treatment, scientific underpinning of the "almost nothing works" doctrine, have appeared thus far; and more are on the way. One critic finds the doctrine an "unanchored, indeterminate, and essentially meaningless statement," not at all suitable as a basis for research, operations, or policy" (Adams, 1975). The other critic, seemingly more skilled at counting than Martinson, notes that at least 48 percent of the studies cited in ECT show either partial or full success in improving the behavior of treated offenders (Palmer, 1975).

A partial or full success count of 48 percent invites comparison with outcomes in other agencies or fields. One of the largest high-technology firms in the U.S. was once described by

an ex-president as getting about 5 percent ultimate payoff from its research projects (Lessing, 1950); a chief of research and development in the U.S. Office of Education told a Senate committee that after $100 million was spent on research in a period of a few years, there was little to show for it (Gideonse, 1967); a Columbia University sociologist, commenting on our waning faith in the capacity of science to solve social problems, asserted that the billions now being spent on cancer research probably would prove to be the most disappointing investment of all (Nisbet, 1975); and a science editor, in a provocative article entitled "Cancer: Now for the Bad News" (Greenberg, 1975), attempted to show in quantitative terms why the research war on cancer was a failure.

These general observations suggest that almost nothing works anywhere -- in corrections, high-technology industry, medicine, education, or what have you. Martinson has merely told us that we can't get rich quickly in correctional research and he has not bothered to inquire as to what the situation is in other fields. If we do make a detailed comparison of research productivity in the several fields, it appears that correctional treatment evaluators are doing, if not better, then at least as well as evaluators in the other fields.

2) Should We Change the Correctional System Rather than the Correctional Client?

This question served as title of a recent article by Bennett, chief of research of the California Department of Corrections (1973). Shortly after, a second article appeared, reporting that research in corrections changed systems more visibly than it changed offenders (Adams, 1974). This is an interesting convergence, but it

need not be highly surprising at this juncture. Indeed, since our initial goals for client change have not been met -- perhaps because they were quite unrealistic -- it may be prudent to shift some resources to the investigation of system change.

There are two rationales for such a shift. First, poorly functioning systems should not be perpetuated. Trial changes in structure or procedure might demonstrate useful variants of the old system or even encourage revolutionary rearrangements. In either case, the old goals might be more effectively achieved under the new designs.

The second rationale is that systems are easier to change than people, and some kinds of system change should be sought as ends in themselves. Etzioni elaborated on this recently (1972) in his article, "Human Beings Are Not Very Easy to Change After All." Those jurisdictions that have ended arrests of inebriates, referring them instead to medical or welfare services, have learned the futility of trying to end public drunkenness through criminal justice measures. The extent to which this lesson applies as well to prostitution, drug use, gambling, and some kinds of sexual behavior now is before the community at large. We may eventually conclude that much of these and other problems now labeled deviant or criminal may be resolved more satisfactorily by focusing on the system rather than the client.

The extent to which criminal justice problems can or should be approached through system change, either as means or as end, is not clear. What is clear is that the treatment evaluator will in the future share resources with the system evaluator -- the

individual who sees systems as candidates for elimination if they persist in working poorly.

3) Better, More Rigorous, Research Methods: It is commonly accepted that the "best" evaluative research is quantitative and rigorous, featuring controlled experimental designs or multivariate analysis. This view has been challenged in recent years, particularly where applied research is concerned. Van de Vall, for example, in a study of research impact in 120 industrial and social organizations, found qualitative research more influential on policy than quantitative research (van de Vall et al, 1975). Adams (1974), reviewing a smaller number of cases of correctional research, reported that there appeared to be no relationship between rigor of design and the impact of the research.

Clarke and Cornish (1972), of the British Home Office, reporting on disappointing results from a time-consuming and expensive controlled experiment at Kingswood Training School, suggested that the experimental method may have a more limited function in penal research than has sometimes been ascribed to it in the past. They stated that its role in corrections was certainly much more limited than in medicine.

Bailey (1966), while examining the efficacy of treatment, also appraised the value of rigor in research. Commenting on the 100 reports of correctional treatment, he noted that increasing rigor was not associated with better results. In his words, despite "impressive evidence of... progressive improvement in the caliber of the scientific investigations conducted... there has been no apparent progress in the actual demonstration of the validity of correctional treatment" (Bailey, 1966:738).

Operations research people enter the discussion from another direction. They note, as in the case of Empey's Silverlake Experiment (1972), that cost-benefit analysis sometimes shows worthwhile savings even though the accompanying experimental design was unable to demonstrate behavioral improvement. More importantly, while experimentation often works well for small, precisely defined problems, policy-makers need increasingly to make decisions about large systems. This turns them toward operations researchers, with their crude but developing capacity for simulating such systems under a variety of circumstances.

There is no easy resolution of this confusion over method. Perhaps the prudent course is to encourage the development of skills -- not necessarily in the same person -- in both traditional and contemporary methods of measurement and analysis, and to develop flexibility in applying whatever methods the time and circumstances appear to call for.

4) Better Correctional Theory: Bailey, who found that his 100 reports demonstrated only that the efficacy of correctional treatments was "slight, inconsistent, and of questionable reliability (1966:738), believed one remedy for this deficit was better correctional theory. Lipton et al, in ECT (1975:627-28), also argued for better theory at the conclusion of their analysis of the 231 correctional reports.

Bailey and Lipton et al offered only general specifications as to the kinds of theory required. Van de Vall, in discussing the impact of research on 120 agencies, introduces some complications. He notes (van de Vall, 1975) that atheoretical research

carries a heavier impact than research based on formal theory, where the latter is defined as general formulations taken from the literature on corrections. However, research based on "grounded" theory proved more influential than atheoretical research.

Clarke and Cornish approach the issue of theory somewhat indirectly, ending in what appears to be agreement with van de Vall. They point to two streams of penal research: the evaluative, which focuses on outcomes; and the sociological, which seeks to explain treatment processes. They find evaluative research disappointing, perhaps because it has been hindered by a poor conception of what goes on in treatment. They call for more research aimed at explanation of processes -- a kind of activity that should in time create a body of low level (grounded?) theory about the nature of treatment and its effects (Clarke and Cornish, 1972).

Glaser is prominent among correctional researchers who see theory as critically important to the programming and evaluation of treatment. His article, "Remedies for the Key Deficiency in Criminal Justice Evaluation Research" (Glaser, 1974), examines theory in relation to offender types. Whereas Clarke and Cornish emphasize the treatment process, Glaser fccuses on differences among groups of offenders.

Palmer and Warren have contributed an even broader perspective on partially tested theory. Offender types, treator types, offender-treator matching, treatment settings have all been brought into a working conceptualization of the treatment effort.

While the results thus far from California's Community Treatment Project have been largely fragmentary (Warren, 1970; Palmer, 1975), the project may be the most notable attempt thus far to develop a comprehensive body of correctional theory in an operational setting.

Since the project appears neither to support nor refute van de Vall's findings about the relative importance of grounded theory, no theory, and formal theory as bases for effective research, we are left with the options of siding with van de Vall or holding out for some more convincing formulation.

Accepting the notion that grounded theory is the best basis for research that will have impact, the question remains as to how one arrives at grounded theory. Does one start with formal theory and by successive trials in operational settings achieve a body of grounded theory? Or does one set out to solve operational problems with no theoretical preconceptions -- if that were possible -- and evolve a body of theory in repeated attempts to develop a solution to the problem?

Furthermore, how does one bring theory to an existing program, previously set up by operating personnel with little attention to either formal or grounded theory? Sometimes the only recourse the researcher has is to organize the final data in some reasonable pattern -- classifying subjects of treatment according to theoretically relevant subtypes, for example.

It is clear that the issue of "better theory" will not be easily disposed of, even if we think only of clients in treatment programs. When we extend the inquiry to the kinds of theory that will be necessary for dealing effectively with system modi-

fication, the issue is likely to stay with us for some time.

5) Location of the Research Effort: The poverty of correctional research is sometimes attributed in part to the location of the research effort. Some academicians assert that research should be pulled out of agencies, where it is dominated by administrators. This impairs its objectivity and subverts it to trivial ends. Glaser, in his discussion of the importance of theory in research, stresses the need of making more research offices independent of the agencies whose programs they are expected to evaluate (1975). Bernstein and Freeman, writing on the relative merits of academic and entrepreneurial research (1975), concluded that research of the best quality was to be found in academe, in departments of psychology, executed by individuals who eventually published in scientific journals. This conclusion was based not on outcomes or impacts of research but on the initial designs.

Van de Vall, in his study of the impact of research in 120 organizations (1975), reported that the research most influential on agency policy was that conducted by agency researchers. This conclusion is of special interest because van de Vall is himself an academician. Adams, a non-academician, conducted a study of 185 criminal justice agencies in which one of the central questions was the sources of useful research. The response was similar to that reported by van de Vall: The most useful research came from the in-house research unit. This was reported not only by state correctional agencies but also by state court administrations and large police departments (Adams, 1976).

Altogether, seven sources of research or research products were ranked by the correctional agencies. After in-house research, the next four choices were "conferences and literature," "research done in other agencies," "consulting firms" (the entrepreneurial researchers of Bernstein and Freeman) and "university researchers."

The findings by van de Vall and Adams suggest that correctional agency research units may deserve more esteem than they have thus far received. Only one academician appears to have accorded them serious recognition. Morris (1972), in an essay entitled "Impediments to Penal Reform," asserted that "California is producing more meaningful evaluative research than any other state or country in the world." The reason is that it has built evaluative research deeply into the administrative structures of the adult and youth correctional agencies. Morris sees the same as true in the United Kingdom, but to a lesser degree.

One of the reasons criminal justice agencies may place such high value on their in-house research units is that we are now in an era of systems change. An in-house unit, which has intimate knowledge of the system, is likely to be more useful in system reconstruction than academic researchers, who are partial to the testing of hypotheses about the effectiveness of particular types of intervention with selected kinds of offenders.

This may also explain the high rank given to consulting firms by police departments and court systems. Such firms tend to focus on problems of organization and management, with no great concern about testing hypotheses derived from formal theory or even grounded theory. These skills appear to meet more readily

the perceived needs of courts and police departments under the rapidly changing criminal justice scene.

Such considerations imply that the location of correctional research activities -- whether in-house, academic, entrepreneurial, foundation, or state planning agency -- is a complex issue. It appears that for the near term there are decided advantages to the building and supporting of capable in-house research units. Further experience with such units may aid considerably in working out effective divisions of labor among the several types of research entities.

6) Orientations of Researchers: Some of the concern about low research productivity has translated itself into questioning of researcher styles. The traditional value-free, neutral stance characteristic of basic scientists and valued by most academicians is undergoing modification as researchers gain experience in operational or action settings.

The applied researcher is the generic younger cousin of the basic scientist. He seeks not new knowledge or theory but the application of knowledge to practical ends. He may do this with some detachment, leaving value choices to decision-makers, but working actively in support of those choices. He may, on the other hand evolve into one of the variants of the applied role.

One of these variants is the advocate -- the researcher who takes a value position and executes research to demonstrate and develop the position. From the standpoint of research tradition, this is risky, since it interferes with objectivity and may lead to erroneous findings and recommendations. However,

times of crisis and innovation seem to place a premium on this researcher style.

A second variant is the apprenticeship model, in which persons with research potentialities but little training in research are taught in operational settings to evaluate their own operations. This model was conceived to avert the great loss of learning that occurs when outside researchers enter agencies on specific project, leave behind final reports, and depart with masses of unreported wisdom that soon dissipate. The apprentice model may be a temporary one, destined to disappear as agencies move increasingly to establish professional in-house research units.

Whether we begin with researchers of the neutralist persuasion or with products of the apprenticeship model, we see a tendency for researchers in agency settings to move gradually toward more pragmatic postures in their work. They begin to understand the administrator's need to act on the best information available at the moment. They also lose some of their inhibitions -- if they ever had them -- against giving the administrator less than full knowledge about an operational problem. Finally, they become more accustomed to the need to fit knowledge to the decision-making tempo, and methodological and reporting compromises appear with increasing frequency. Relative ability to make these pragmatic adjustments undoubtedly influences the comparative rankings of entrepreneurial and academic research in some sectors of the criminal justice arena.

7) <u>The Training of Correctional Evaluators</u>: Observations

such as these prompt questions as to how agency evaluators should be trained. If they are to become in-house researchers, they will be more confortable if they do not become too deply imbued with pure science orientations and expectations. And if they aspire to become consulting firm staff members, they may do well to learn problem-solving approaches, with skills in the newer analytical and measuring techniques.

Philip Abelson, editor of Science, once observed that industrial organizations occasionally showed some aversion to academic Ph.D.s as research recruits (1973). They preferred to bring in promising individuals at sub-doctoral levels and train them in the organization's research unit. This reduced the number of individuals in the research unit who wanted to keep on re-doing their theses indefinitely.

What we are witnessing here is increasing role differentiation, with in-house and academic researchers differing in significant ways, and entrepreneurial researchers showing still other characteristics. Several implications are evident here. Researcher training must allow for wider career variety; the choice of research as a career presents decisions of greater complexity; communication among established researchers loses some of its simplicity; and the administrator has more difficult judgments to make when he seeks to use research as an instrument of management and policy.

8) Role of the Administrator: A final source of uncertainty in correctional evaluation is the changing role of the administrator in relation to research and researchers. There is a

common conception of the agency director as a person who values research only if it supports his programs and objectives. Campbell (1969) has described him as someone "trapped" by his emotional commitment to hoped-for outcomes, and inclined to reject or deny research findings that conflict with those outcomes.

Administrators of this description exist. But like the researchers who become pragmatic under exposure in the arena of decision-making, some administrators have learned to adopt the experimental stance. They develop a commitment to problem-solving, not program perpetuation. Glaser (1973) offers an illuminating description of administrative policy toward evaluation in the California Youth Authority. The agency director proceeds on the assumption that new ideas are to be tried provisionally, with rigorous evaluation, and the continuation of the innovation depends on its showing under evaluation.

This is acquired behavior. When the Youth Authority's research division was established in 1958, the Director and his Chief Psychiatrist both opposed a controlled experimental design to evaluate the effectiveness of psychotherapy with disturbed older juvenile wards. They asserted that it was unthinkable to deny therapy to anyone judged in need of it. Furthermore, the treatment staff would not permit it. Several months later, after a quasi-experimental design had produced inconclusive data, thus casting doubt on the efficacy of psychotherapy with these wards, both the Director and the Chief Psychiatrist readily changed their stands and allowed a rigorous

controlled experimental design. An open policy toward research persists to this day in the Youth Authority.

Over the past twenty-five years, approximately two-thirds of the state departments of correction in the U.S. have set up in-house research units. Most of these are relatively small -- quite unlike the research division of the California Youth Authority, which reported 43 professional researchers last year. However, the smaller units draw frequently on the experience and the research products of the larger. It seems quite likely that one of the unofficial functions of many of these units is to move their departmental directors along the road from the "trapped" stance toward an experimental, problem-solving orientation. Since there are other influences in directors' careers that operate in the same direction, it is not clear just how important in-house units might be in the research education of departmental directors.

Discussion

At this point, I can only guess as to whether uncertainty as to the future of correctional treatment evaluation has been diminished or increased. Before we inquire into that point, let me comment further on several matters and then move to a summing up.

Let's turn first to the matter of whether anything works in correctional treatment. Martinson's spirited insistence that almost nothing works is stimulating interest in punishment and incapacitation. There seems to be a broad movement among law enforcement officials, prosecutors, judges, and even

some correctional administrators away from rehabilitation as a major goal. This movement is probably premature. And it seems significant that most correctional administrators, though shaken by Martinson's thesis, have not yet jumped on his bandwagon. Serrill's survey (1975) discloses that they still maintain faith in their ability to rehabilitate some offenders.

This first reaction against the nothing works doctrine may accelerate, for at least two reasons. First, the formal critiques of the doctrine have begun to come in (Palmer, 1975; Adams, 1975), and they seem to argue effectively against Martinson's position. They do not establish that <u>everything</u> works. Furthermore, they must struggle against the contemporary tides of social pessimism and criminal justice conservatism. Nevertheless, the search for what works best under what circumstances in correctional rehabilitation is likely to continue and to show further achievements.

Second, the likelihood that significant social gains will be made, either in research or in policy, by pursuit of punishment or incapacitation as the dominant correctional goals is questionable. In selected instances there may be identifiable achievements, but the odds for progress through punishment are inherently poor. When only a very small percentage of offenders out of the total universe of offenders experiences punishment under present systems, a shift from rehabilitation to punishment seems a desperate gamble. The gamble is not helped by the fact that our grasp of the theory underlying punishment or deterrenee of offenders is far weaker than our knowledge of the theory of

rehabilitative treatment.

While the effect of the Nothing Works doctrine is likely to be only temporary, the consequences of efforts at systems change are bound to be more lasting. The Probation Subsidy Act of California or the youth institution closings in Massachusetts are cases in point. They mean the end of opportunity for institutional program evaluations for sizeable numbers of offenders in those states. This may be more important in principle than in practice since only a small part of the national institutional population is involved. Furthermore, the probation subsidy seems to be having difficulty migrating from California to other jurisdictions, and the Massachusetts institutional closings have not yet become a nation-wide epidemic.

Nevertheless, agency administrators continue to show strong interest in system change. And during the time that this interest remains strong, many correctional researchers will find themselves working on problems relating to system modification rather than client modification.

System concerns call for shifts in research methods and strategies. In their paper, "Criminal Justice Sets, Strategies and Component Programs: Evaluating Change in the Criminal Justice System," Coates and Miller (1975) contrast the system model approach with the more traditional goal model approach. They use as example the system model they developed to study the new situation in Massachusetts youth corrections. They stress the superior capacity of this model to deal with the changing character of the criminal justice system and with the conflicting

goals pursued by different interest groups in the community.

Coates and Miller, like van de Vall, do not declare obsolete the controlled experimental design or other traditional research techniques that have long been held in high esteem. They argue rather that the stage has broadened and that there is now required greater variety in methods, models, and strategies.

- This broadening of the stage has its implications for theory as well as method. There is not only a proliferation of psychological and sociological schools to be found among correctional theorists; there is an expansion of theoretical activity to include systems theorists and perhaps economic theorists as well -- although the latter discipline tends to make its major contributions in methodology, as cost-benefit analysis and deterrence research will attest.

To these internal developments in correctional research, we can add the organizational restructuring of the activity. The most conspicuous aspect of this is perhaps the rise of the in-house research unit. Practically non-existent thirty years ago in state departments of correction, such units now are to be found in about two thirds of these departments. This is not merely a correctional phenonenon, since there has been an even faster rise of in-house research units in police departments (of larger size) and state court administrations. The earlier involvement of departments of correction in research may be attributed to the initiative of university psychologists and sociologists, who found prisons convenient laboratories for the

study of the prison community and the effects of prison programs on offenders. The rise of the in-house unit has diminished the role of outside researchers. University researchers, who may be assumed to have been the primary source of correctional research at mid-century, are now ranked fifth as sources of useful research. This does not rule out the possibility -- or perhaps the probability -- that university researchers are still the primary source of basic research on offenders and their response to treatment.

What kind of division of labor will eventually emerge between academic, in-house, and entrepreneurial research is conjectural. Academic researchers must be valued for their sensitivity to the importance of theory in programming and evaluation, the capacity for objectivity implied in their location, and their strategic role as trainers of new classes of correctional researchers. Entrepreneurial researchers may be recognized for their practical command of newer analytical techniques, their wide-ranging experience, their commitment to problem-solving, and their business-like approach to the task at hand. In-house researchers have the advantage of familiarity with the site and substance of the problem, ease of communication with administrator or practitioner, relatively low cost, effective working relationships with planners and developers, and an opportunity to make research in a particular organization accumulative.

Conclusion

A discussion that sets out to examine some of the uncertainties in correctional research hopefully does not end where it

began. Perhaps we can conclude by reiterating a few points that may serve as temporary guides in a time of transition.

First, it seems likely that correctional treatment has been and can be more efficacious than we have recently been led to believe. A fair assessment of the work of correctional evaluators -- were such an assessment readily possible -- would probably show them disclosing results not at all inferior to most other human enterprises. There now remains for administrators and politicians to consolidate and implement these disclosures. And in deciding what deserves implementation, we need to listen more to the Palmers than to the Martinsons.

One encouraging note in recent developments is the rising interest in system reconstruction. This is no less important than continuing interest in offender rehabilitation. Hopefully, with adequate attention to system matters, we can not only provide a better setting for new rehabilitative initiatives; we can also cause part of what is called the "crime problem" to disappear.

These ambitions pose new problems for both old and new methodologists and old and new theorists. Many of our assumptions about research methods may need review, and what works in research must be recognized as equally important as what works in correctional treatment. It is reassuring that we are witnessing a trend toward evaluation of evaluations, and out of this should come sharper perceptions on how to structure our research enterprises. As a companion effort, we need further scrutiny of the kind reported to us by van de Vall -- the relative efficacy of formal theory, absence of theory, and grounded theory as bases for productive research. This constitutes an essential element in

our overall task.

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